

January 28, 1952

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Dear Ed:

Thank you very much for the 679 culture, and your letter of the 16th with its invitation. Both arrived in good condition, and most welcome.

I don't know whether you meant very seriously your last paragraph about discussing your symposium session; I will assume that you know me well enough to realize that I would. First of all, I regret that I have not planned to attend the SAB meetings this year. Last summer's Cold Spring Harbor session left me with very little reserve supply of meeting energy, and I am afraid that whatever has reaccumulated in the meantime will be drained by the Cincinnati Cancer Congress the first week in March (which has my promise not to say anything about the implications of microbial genetics for cancer research). I shall be very disappointed if this date conflicts with your Enzyme Seminar, but perhaps there will be a reinforcement rather than a conflict.

Frankly, I wonder if "this somewhat ambitious program" is not going to turn out to be an understatement, when looked at after its two-hour lease on life. I would imagine that a very successful session could be constructed around any one, or perhaps two, of the suggested topics—the first and second. This reaction is colored, even jaundiced, by last summer's experience, so perhaps you and your audience won't have a similar response. But I honestly think that a more circumscribed subject would be more successful in the end. I won't presume to suggest additional discussants for your provisional 1st and 4th topics (except to mention that Wainright, who has done some beautiful work on enzymatic adaptation with Pollock, is in Ryan's lab for the while). Re 2., Werner Braun has given some (almost surprisingly) excellent talks on his recent work. For 3., if you don't want to do this yourself, Newcombe has had the next most experience with recombination analysis. Zinder will be at the mtg, but I don't ~~want~~ to encourage (or discourage) singling him out for such a panel until he's had a little more experience. Hattie Alexander has, of course, another interesting transduction story in *Haemophilus*. Do you know that Delamater has any genetic confirmation of his copulation story? He promised to wire me when and if, but not yet. A whole session could easily be devoted to a debate on his mitosis cytology.

A word about science:
F-(self incompatibility) crept in between 679 and 679-680. The transmission of F+ (e.g. from 58-161) is remarkable: it is infective, but distinct from lambda. The factor has not been filtrable, but modifies most of the F- cells grown with an F+ culture, and later separated by various markers.

Sincerely,

Joshua Lederberg

*know whether